# NORTHWEST POWER PLANNING COUNCIL/ NATIONAL MARINE FISHERIES SERVICE TECHNICAL FORUM ON PATH

# MEETING NOTES

**February 25, 1999, 9:00 a.m.-5 p.m.**

**NORTHWEST POWER PLANNING COUNCIL OFFICES**
**PORTLAND, OREGON**

## I. Greetings and Introductions.

The February 25, 1999 NWPPC/NMFS technical forum on PATH, held at the Northwest Power Planning Council's offices in Portland, Oregon, was chaired by Todd Maddock of the Council and facilitated by Donna Silverberg. The agenda for the February 25 technical forum and a list of attendees are attached as Enclosures A and B.

The following is a distillation (not a verbatim transcript) of items discussed at the meeting, together with actions taken on those items. Please note that some enclosures referenced in the body of the text may be too lengthy to attach; all enclosures referenced are available upon request from NMFS's Kathy Ceballos at 503/230-5420 or via email at kathy.ceballos@noaa.gov.; all of the overheads used by participants in today's forum are available upon request.

## I. Introductions and Review of Agenda.

Maddock welcomed everyone to the meeting. He thanked the PATH participants for all of their hard work, observing that the PATH process has been long, valuable, and not without its controversial aspects. Our goal today is to address some of the questions and controversies surrounding PATH, Maddock explained; it is our hope that this forum will give us all a better understanding of PATH's accomplishments, and of the work that remains to be done. The focus of today's meeting will be PATH's report on its 1998 activities, and what those activities and results tell us; this forum is intended as an opportunity for the public to ask questions and offer comments on those results to date.

Maddock explained that PATH was formed as a result of the 1995 Biological Opinion, and a desire to resolve differences between the various computer models used by federal, state and tribal agencies. The PATH membership includes 30 scientists from federal, regional, state and tribal institutions, as well as independent scientists.

The agenda for today's forum was set by questions submitted from throughout the region, Maddock continued. There were more than 100 questions submitted; they were distilled into nine major questions that will be the starting-point for today's discussion. Maddock then introduced Donna Silverberg, the facilitator for today's forum, who went over the ground rules for discussion.

In response to a question from Bruce Lovelin of the Columbia River Alliance, NMFS' John Palensky said the minutes for today's forum will be sent to everyone on the Implementation Team mailing list, as well as those who sign up to receive them at today's meeting. In response to another question, he said NMFS has not planned any additional public meetings on the PATH process at this point; additional meetings will depend, in part, on the outcome of today's session.

PATH, of course, represents the biological side of the drawdown analysis, said Jim Nielsen of WDFW -- does NMFS have plans to hold a similar public forum on results from the economic side of the analysis, which is being conducted by the Drawdown Regional Economic Workgroup (DREW), particularly in light of recent comments from the Independent Economic Analysis Board (IEAB) on the validity of the DREW work products? IT Chairman Brian Brown replied that DREW's activities are a part of the Lower Snake Environmental Impact Statement (EIS) process; there will be opportunity for public input once the draft EIS is published, but we had not planned anything prior to that point, said Brown.

Silverberg led a review of today's agenda, then introduced PATH facilitator Dave Marmorek.

**II. Introduction to PATH Decision Analysis**.

Marmorek introduced the PATH scientists present at today's meeting, then set the stage with a few introductory comments regarding the PATH process and structure, the actions that have and have not been considered to date, the current status of the PATH analysis, the structure and limitations of today's session, key terms, and PATH's planned activities for 1999.

Marmorek noted that all PATH work products are reviewed by the Independent Scientific Review Panel (ISRP); their reviews are submitted to the Implementation Team and the Northwest Power Planning Council. He described the mechanisms by which the PATH tasks are formulated, prioritized, accomplished and distributed.

Most of the people in this room are aware that there are a lot of uncertainties associated with this problem, Marmorek said – in fact, there are more things we don't know than things we do know. These uncertainties have generated various alternative hypotheses and assumptions about how nature operates, and how nature may respond to various human actions. PATH went through an intensive weight-of-evidence process on spring/summer chinook last year; many of the results that will be presented today came out of that weight-of-evidence process. Basically, we specified alternative hypotheses related to flow and survival, transportation, drawdown etc. – and simulated them through modeling processes. We then looked at those results, Marmorek explained, and said, which of those uncertainties really matter, and which don't seem to make much difference? For those that did seem to matter, we then put together all of the evidence for and against the alternative hypotheses in a weight-of-evidence report.

The ISRP provided their independent, subjective judgements on those alternative hypotheses, Marmorek continued; we then applied the weights the ISRP generated and compared that to how things might turn out if we weighted all of the alternative hypotheses equally. As a result of that exercise, the ISRP strongly recommended that we design management experiments to tease out some of the uncertainties, he said; their conclusion was that more modeling is unlikely to resolve some of the key unknowns.

The alternatives PATH has looked at to date include A1, basically a continuation of current operations; A2, under which transportation would be maximized using the existing set of bypasses and collection facilities in the system; A2', which also maximizes transportation, but adds surface bypass collectors; A3, drawdown of the four Lower Snake River dams with no transportation; A6, the in-river alternative (no transportation, additional bypass improvements at the dam sites); A6', which is the same as A6, but with reduced flow augmentation; and B1, drawdown of the four Lower Snake dams plus drawdown of John Day Dam.

Marmorek said PATH's current number one priority is refining its preliminary fall chinook analyses; most of today's discussion, however, will focus on spring/summer chinook results. There have been a number of additional sensitivity analyses proposed for spring/summer chinook; how much of that additional work PATH can accomplish in FY'99 remains to be seen, Marmorek said. As I mentioned, the ISRP has strongly recommended that we investigate experimental management, to see what kinds of experimental manipulations could be done to improve understanding and decrease uncertainty, he continued; that is another task on PATH's plate, and it will be up to the IT to make a decision about the relative importance of experimental management. Finally, said Marmorek, PATH is also looking at scoping out the volume of work involved in doing analyses of Mid-Columbia steelhead and spring chinook in FY'99.

Marmorek touched on the structure of today's meeting, noting that the information to be presented is quite complex. He then introduced Randall Peterman, who spent a few minutes describing the decision analysis approach used by PATH.

Peterman explained that the overall goal of the PATH process is to identify ways to maintain and recover listed salmon stocks; however, as everyone is aware, the actions required to accomplish that goal are unclear, first, because this is a relatively new situation, second, because of the large number of uncertainties associated with this task, arising from the complete lack of certain types of data. The data we do have includes considerable natural variability, because conditions are different from year to year, Peterman said; we are also hampered by imprecise observations and the fact that several simultaneous events (changes in the flow regime, the introduction of hatchery fish to the system, climate change) confound the observations we do have. These uncertainties will always be present; nevertheless, decisions have to be made.

How, then, can we make the best decisions possible, in the face of these uncertainties? Peterman asked. First, we can use research to reduce the uncertainties, he said. The PATH retrospective analysis did exactly that, taking the available evidence and trying to squeeze as much information as possible. Second, we can do a decision analysis, which takes into account the uncertainties we know about, and ask what effect they have on the rank order of the available alternative actions.

So what is decision analysis? Peterman asked. It is a framework for evaluating the merit of different management options, which explicitly takes uncertainties into account. It is a well-proven technique, which has been used in business for more than 30 years. No method is perfect, but decision analysis has proven its worth as a way to provide insight, he said.

The decision analysis approach employed by PATH has eight major components, Peterman said. First, the definition of a management objective – what we want to achieve – and performance measures to reflect how well it is achieved. The NMFS survival and recovery standards are the measure of performance PATH is using to determine how appropriate a particular management action is.

Second, Peterman continued, those alternative management actions are defined; the ones PATH is using were described by Dave Marmorek a few minutes ago (A1, A2, A2' etc.). The third component of decision analysis is the articulation of the relevant uncertainties or key hypotheses – for example, the flow/survival rate relationship, the effects of transportation and the influence of climate changes. The fourth component of decision analysis is to assign probabilities, or weights, to those key hypotheses.

The fifth component of the decision analysis process is to develop a modeling framework to calculate the outcomes for each alternative action, and for each hypothesized state of nature. Basically, what we're asking is a set of "what-if" questions, Peterman explained – what if this hypothesis is true, and, if so, what are the effects of a given management action? PATH is using various models, including CRiSP, FLUSH and a full life-cycle model, to get at that information.

The sixth component of the decision analysis is the generation of a decision tree, a condensed, schematic version of the real decision tree. The decision tree lays out, in graphic form, what the alternative actions are, and their connections with all of the alternative hypotheses that were considered. The seventh task in this process is to go through the calculations of each outcome, and weight the outcomes that were calculated based on some scheme – in our case, either an equal weight assigned to each hypothesis, or the weights developed by the ISRP. From that, PATH can calculate the expected probability of reaching a NMFS survival or recovery standard. The alternative actions are then ranked according to that expected value, Peterman explained.

The final, and perhaps most important, part of the decision analysis process is the sensitivity analysis, he continued. Basically, what we do in this process is ask how the rank order, and the probability of meeting the NMFS survival and recovery standards for each alternative action, changes when we change assumptions.

The PATH decision analysis is by far the most comprehensive example of applied decision analysis for any resource management problem that I know of, Peterman continued. It is an extremely complex undertaking, he said, but if anyone can suggest a better approach, I'd like to hear about it.

**III. Assumptions**.

**1. Upriver vs. Downriver: Can Lower Columbia River Populations Serve as Controls for Upriver (Snake River) Populations?**.

To answer this question, said Marmorek, first we have to define what we mean by "controls." There are seven Snake River index stocks and six Lower Columbia River index stocks. PATH's 1996 report identified two important patterns, he said. When we compared recruits per spawner – how many adult salmon return to Bonneville Dam compared to the number of spawners that produced that generation – we found, first, that recruits per spawner were lower in the 1975-1990 period than they were in the period 1949-1969. That was true throughout the region – for the Snake, Mid-Columbia and Lower Columbia species, Marmorek said. The other pattern that emerged from this analysis was the fact that there were larger declines in the Snake River recruit-to-spawner ratios than in the Lower Columbia recruit-to-spawner ratios. The analysis doesn't say anything about why that happened, Marmorek said – it simply identified the patterns.

When we talk about using the lower river stocks as controls for the upriver stocks, that implies that both groups of stocks have similar estuary and ocean survivals, Marmorek continued. PATH considered two alternative hypotheses – first, that the downriver stocks are partial controls, because the upriver and downriver stocks have similar survival in the estuary and ocean (the "common-year effect"), and that regional differences in the recruit-to-spawner ratio are less-likely to be attributable to ocean conditions, and more likely to be due to migratory corridor conditions and/or differences in productivity (the "Delta" hypothesis).

The second hypothesis PATH considered was that downriver stocks are not controls for the upriver stocks, Marmorek said – that upriver and downriver stocks have different survivals in the estuary and ocean, and there is no common-year effect (the "Alpha" hypothesis). This hypothesis assumes that ocean conditions play a major role in regional recruit-per-spawner differences.

Marmorek went through a sample analysis, using hypothetical numbers, to show how these hypotheses are applied, and their respective effects on both upriver and downriver stocks. In terms of the implications of this phase of the analysis, Marmorek said, when the common-year effect is included, we generally end up with higher forecast escapements for both upriver and downriver stocks. Using one or the other of these hypotheses doesn't change the ranking of the alternatives, but it does have a small effect on their likelihood of meeting the NMFS recovery standard. The "Delta" model shows slightly higher average probabilities of escapements meeting the survival standard, but it doesn't change the relative ranking of Alternatives A2 or A3. The "Delta" model has a somewhat larger effect on the likelihood of a given alternative meeting the 48-year recovery standard, but again, it doesn't change the ranking of any of the alternatives, Marmorek explained. The "Alpha" approach tends to show a step change in extra mortality, around 1977, while the "Delta" approach shows a more gradual change in extra mortality, Marmorek added.

At Marmorek's request, PATH participant Charlie Paulson spent a few minutes describing the evidence supporting the "Alpha" approach; PATH member Charlie Petrosky did the same for the "Delta" approach. Both Paulson and Petrosky worked from a series of overheads, attached as Enclosures A and B, respectively; please see these documents for details of their presentations.

At the end of these presentations, Rob Walton asked to what extent consensus exists as to whether or not the downriver stocks can be used as controls for the upriver stocks. We recognize that there is some uncertainty associated with how good a control the downriver stocks are, Marmorek replied; that's why we've incorporated these two different hypotheses – to see how big a difference the two assumptions make. Obviously the downriver stocks aren't perfect controls, added Paulson.

Another participant, from Idaho Rivers United, said that, as he understands it, the CRiSP model assumes that whatever has caused the decline in the Snake River stocks is unrelated to the hydrosystem, occurred after the construction of the Lower Snake River dams, occurs outside the migration corridor, and whatever this influence is, it selects for the Snake River stocks. My question, he said, is whether or not PATH has pinpointed any factor that might fit this description? Also, what did the ISRP have to say about that hypothesis?

PATH member Jim Anderson replied that neither the CRiSP model nor the FLUSH model really say anything about what happens outside the hydrosystem. What CRiSP does is characterize the system down to Bonneville Dam; once you've done that, you've identified a level of extra mortality, and can then look for hypotheses that explain that extra mortality, he said. The CRiSP model doesn't deal with those things, Anderson said, so your understanding that CRiSP is saying something about the ocean is not correct. As far as the reasons for the extra mortality, said Anderson, if it exists, I think it's fair to say that we do not have a good mechanistic basis at this time to explain what's causing it, nor do we have a good mechanistic basis for understanding how the removal of the four Lower Snake dams might improve the system.

In terms of the other part of his question, said Petrosky, the ISRP did not find that there is evidence for differential ocean survival for upriver and downriver stocks independent of the effects of the hydrosystem.

**2. Post-Bonneville Survival: What Was Assumed for Post-Bonneville Survival? What Is the Rationale, and What Are the Biological Implications? Were Decadal and Annual Variations in Climate and Ocean Survival Accounted for in PATH?**

This question obviously has several facets, said Peterman; it gets to the heart of the extra mortality hypothesis, as well as the question of how the climatic processes are represented in the PATH analysis.

First of all, said Peterman, what do we mean by "extra mortality?" When we refer to the "extra mortality hypothesis," we're referring to the mortality of non-transported juvenile fish which occurs outside the migration corridor, and is not accounted for by stock productivity (recruitment) estimates, direct mortality estimates from the in-river passage models, or year effects common to all stocks.

In order to explain how the extra mortality hypotheses were formulated, Peterman continued, I need to describe the assumptions PATH made about future climate. PATH considered several alternative hypotheses about the effects of climatically-driven oceanographic changes, Peterman said. These include:

- **A. Between-Year Variation** (sampled from past variation in the Delta model; Alpha model assumes an environmental mechanism)
- **B. Longer-Term Variation** (sampled from past variation in the Delta model, with the assumption that good years tend to follow good years and bad years tend to follow bad years; Alpha model assumes a 60-year repeating cycle)
- **C. Spatial Differences Among Stocks** (Delta model assumes all stocks affected similarly; Alpha model assumes Snake River stocks affected more by oceanographic conditions than other stocks)
- **D. Downstream Migration Model** (assumptions used in the CRiSP and FLUSH models affect how much mortality is yet to be accounted for by some other mechanism, such as ocean conditions)

Peterman said PATH is using five hypotheses to explain what may be causing the extra mortality:

- **EM1**: indirect effects of passage through the hydrosystem (delayed arrival in estuary, stress from crowding/injury/descaling, increased vulnerability to predators)
- **EM2**: lower stock viability due to BKD, other factors
- **EM3**: 60-year cyclical regime shifts in climate (changes in ocean temperatures, predator/prey interaction, changes in rainfall, snowpack, river flows)
- **EM4**: hatchery fish negatively affect wild fish (competition for food, stress, predation, disease)
- **EM5**: EM1 to EM4, plus bird predation in the estuary.

Peterman described how these various hypotheses are implemented within the PATH analysis, then moved on to the implications of the results: while EM1, EM2 and EM3 all make a significant difference to projected future escapement, no matter which extra mortality hypothesis is applied, they do not change the rank order of the alternative actions (A1, A2 A3 etc.). However, they do affect the ability of the alternative actions to meet the NMFS jeopardy standards.

Peterman said the sensitivity analysis for EM5 (EM1, EM2 or EM3 plus incremental mortality from bird predation in the estuary) implies that

- Incremental mortality (estimated at anywhere from 5% to 40%) does not change the rank order of Alternatives A1/A2, A3 or B1
- Incremental mortality lowers the ability to meet the NMFS jeopardy standards, with a greater effect on A1/A2 than A3/B1.

Next, Howard Schaller discussed the evidence for the EM1 extra mortality hypothesis, which attributes extra mortality to the hydrosystem. Specifically, he said this evidence includes hydrosystem-caused delays in the critical timing of the entry into seawater for juvenile fish; stress during bypass and holding, which increases vulnerability to predation and disease; lower rates of delayed mortality for fish that avoid bypass systems at dams; the fact that productivity and survival rates for all stocks above Bonneville were stable from 1939 through 1970, despite considerable climatic variation, then declined; and the fact that the productivity and survival rate index declined more in the Snake River stocks than in the Lower Columbia River stocks when the pre-1970 and post-1975 periods are compared, which is consistent with the development of the hydrosystem.

The bottom line is that the Scientific Review panel's weight of evidence process weighted the hydrosystem extra mortality hypothesis slightly higher than the other hypotheses, and equal to the BKD extra mortality hypothesis, Schaller said. The SRP unanimously assigned lower weights to the regime shift hypothesis, he added.

Chris Toole of NMFS described the evidence for the BKD hypothesis (EM2) and EM4, the hatchery fish hypothesis. This evidence includes:

- BKD transmitted to wild Snake River fish from hatchery fish will cause mortality with or without the dams
- Extra mortality is correlated with the number of hatchery releases
- The larger number and biomass of hatchery fish may reduce the carrying capacity of the migration corridor, limiting the growth and energy reserves of wild spring/summer chinook
- Hatchery fish increase stress in forebays, possibly increasing disease and predation mortality on wild fish
- Hatchery fish more strongly affect Snake River wild chinook due to the longer distance of co-migration and greater co-mixing of hatchery and wild fish.

The bottom line, said Toole, is that PATH can explicitly model the hatchery hypothesis; however, because of the confounding effects of dams, transportation, climate and hatchery effects, it's difficult to know, if you remove some of the hatchery influence without removing the dam influence, whether that would reduce extra mortality.

Rich Hinrichsen, a BPA fisheries research consultant, then discussed the evidence for the regime shift extra mortality hypothesis (EM3). He emphasized that the quality empirical needed to verify any of the extra mortality hypotheses does not exist; there is a great deal of uncertainty surrounding the actual cause of the extra mortality.

However, Hinrichsen said, if you look at the post-Bonneville survival of in-river fish using the FLUSH model, there really is no extra mortality – extra mortality stayed pretty constant from 1950 through 1990, and most of the mortality can be accounted for within the hydrosystem. The CRiSP model, on the other hand, shows better survival for juvenile migrants during the 1970 to 1990 period; to explain the decline in the number of spawners, it is necessary to define an extra mortality mechanism. In other words, Hinrichsen said, extra mortality applies only to the CRiSP model.

The evidence for the regime shift hypothesis includes:

- The fact that, according to recent research by the University of Washington, Pacific salmon catches in Alaska have varied inversely with salmon catches on the U.S. and Canadian west coast during the past 70 years. The rise and fall in salmon populations in these geographic areas correlate with observed climate indicators; generally, when the Alaska stocks are up, the west coast stocks are down, and vice versa.
- Other factors (dam and hatchery influences etc.) may confound the regime shift hypothesis.

PNUCC consultant Jim Litchfield asked whether the fact that the extra mortality hypotheses apply only to CRiSP and not to FLUSH may be an artifact of the fact that the two models have dramatically different in-river survival estimates. Also, how do the two models calibrate the most recent survival data, and is there an argument that some of the older survival information from the 1960s and 1970s may not be of the same quality as more recent survival data? Marmorek said Litchfield's second question will be addressed in detail during the "Strengths and Limitations" item later in today's agenda. With respect to your first question, said Schaller, in looking at how many fish were transported over that same period of time, most of the differences in the extra mortality component could be attributable to a) the number of fish transported and b) the transport model used. Another component would be what level of in-river survival is estimated. In other words, he said, it's not just the passage models themselves – it has to do with the number of fish transported, the timing, and what you assume about the ratio of survival in the transported vs. the in-river fish.

Has PATH examined the hypothesis that the extra mortality may be a natural phenomenon in some populations, and what proportion of extra mortality might be attributable to natural, rather than man-made causes? asked John Pisementi of HARZA. The reason I raise this question is that many species have sparser populations at the edge of their migratory ranges, he said.

Al Giorgi replied that the modeling exercises that have been conducted in support of the extra mortality analysis haven't really considered the possibility that all of these mechanisms could be acting in concert to depress these stocks. It has been suggested that PATH try to model that, he said, but I don't think we can, because I don't think the information is out there to pick it apart. Schaller added that the different index stocks used in the analysis take into consideration the different possibilities of an intrinsic growth and density dependence, and the extra mortality is just a residual of that. The underlying productivity functions for these index stocks take into consideration the extremes of the range.

The Corps' John McKern observed that system survival in the FLUSH model is very low, while system survival in the CRiSP model is somewhat higher. However, in 1998, the NMFS empirical data for PIT-tag studies shows that system survival is actually substantially higher than is being forecast in either model, McKern said. I would like to point out what I see as a flaw in the PATH process, he said – that is, that the PATH group has used old data to come up with lower predictive values than the values that are now being empirically observed. What they should do is look at this as an evolutionary process, and use the latest survival data available to re-run their analysis. Based on the NMFS results, will the PATH process be updated and re-run, and will any attempt be made to correct the public perception that survival through the hydrosystem is extremely low? Marmorek replied that the substance of McKern's question would be dealt with under the "Results" section later in today's agenda. To briefly answer John's question, said BPA's Jim Geiselman, PATH has proposed to do some additional sensitivity analyses and updating of its analyses in FY'99; this work will be prioritized by the IT.

One final word on Question 2, said Randall Peterman: within the decision analysis framework, one of the criteria for evaluating how important different hypotheses are is how much difference they make to the rank order of the management options. None of the extra mortality hypotheses – EM1, EM2, EM3, EM4 or EM5 – change the rank order of the alternative actions; the drawdown alternative has the highest probability of reaching the survival or recovery standards, no matter which extra mortality hypothesis is applied.

## 3. Factors Affecting Juvenile Salmon Survival Through the Federal Hydropower System.

Marmorek provided a brief introduction to this agenda item, explaining that the key uncertainties and issues associated with this question include the relationship between juvenile survival and flow (travel time), the causes of the low observed survival in 1973 and 1977 and their implications for future predictions of in-river survival.

PATH is using the CRiSP and FLUSH passage models to get at these questions, Marmorek continued; CRiSP has a weak relationship between flow and survival, and attributes the low survival in 1973 and 1977 to passage conditions at that time (debris, descaling etc.), while FLUSH has a stronger relationship between flow and survival, and attributes the low survival in 1973 and 1977 to low flow as well as passage conditions.

Some of the implications of this analysis include:

- Direct passage survival (the average of transported and non-transported fish) is relatively similar between the models for all actions
- Which passage model is used has a large effect on projected spawners and jeopardy probabilities, with the CRiSP results consistently higher than the FLUSH results. This is due mainly to differences in the transportation assumptions used in the two passage models.
- The rank order of the alternative actions is not affected by which passage model is used.

Marmorek then introduced Jim Anderson, who discussed some of the evidence for and against the validity of the CRiSP model. He explained that river survival, which the passage models are designed to measure, is very important – it has a chain of effects down through the system. River survival affects transport survival directly; it affects our estimates of extra mortality; it affects the probabilities associated with Alternatives A2 and A3. In general, Anderson said, all of the hypotheses on extra mortality are not as important if there is indeed a strong flow/survival relationship.

Anderson spent a few minutes describing some of the additional differences between the two passage survival models, and the evidence for and against a strong flow/survival relationship. Overall, said Anderson, the newer data is suggesting that there is a weak flow/survival relationship; there are serious problems with the older data, which imply that the flow/survival relationship is stronger.

Next, Paul Wilson of CBFWA discussed some of the misconceptions about the FLUSH model, and how its output was used in the prospective life-cycle model; he explained that FLUSH is intended to model the general patterns of survival under different environmental conditions, and was specifically designed to be used in prospective life-cycle modeling. In general, the Scientific Review Panel concluded that the FLUSH mechanism makes more sense than the CRiSP model, Wilson said; the model is simpler, and the SRP also provided some additional evidence in favor of the FLUSH model. Some of the pro-FLUSH evidence includes:

- The fact that the negative effects of longer travel times on survival (due to physiological factors, predation etc.) has been documented
- The incremental mortality of Snake River fish is positively related to water transit time
- The reservoir survival estimated from all reach survival data has been agreed on by the PATH subgroup used for this work.

Another reason the FLUSH model was given a higher weight by the SRP is the fact that FLUSH fits all the data well – not just the reach survival data, but the smolt-to-adult return and stock recruitment data as well, Wilson said – in other words, it is more robust than CRiSP.

Wilson talked briefly about how the FLUSH model was implemented in the analysis, then noted in closing that, even under the most optimistic assumptions used in the modeling effort, Alternatives A1 and A2 fail to meet the NMFS jeopardy standards under most hypotheses.

If in fact 2%-6% is the appropriate smolt-to-adult return rate during a wet cycle, asked Mike Field, what is an appropriate SAR during a dry cycle? You're correct that one of the things that went into the identification of 2%-6% as an appropriate SAR is data from prior to the completion of the last four dams in the system, Wilson replied. However, another piece of information that went into that SAR goal was a set of SAR data from the Warm Springs River, which is above two dams, from the period 1975 to 1992. Do you know what the smolt-to-adult return rate should be during a dry cycle? Field asked. We don't have a lot of control stops, Wilson replied, but I believe the average was about 3.5%.

One thing about smolt-to-adult return rates, added Schaller – if you're looking at what you need to meet the NMFS jeopardy standards, it doesn't matter whether it's a wet cycle or a dry cycle. You're still going to need the same average improvement in SARs. The smolt-to-adult return rate is the underlying survival, and all we're saying is, no matter if it's dry or wet, in order to achieve the NMFS standards over a given period of time, you need the same rate of improvement in the SARs.

You're correct when you say that the 2% number came about from analyzing the historic SARs from the early period, said Marmorek. The other thing we did was look just at the life-cycle models and said, if we achieve these standards, what is the average smolt-to-adult return rate that is needed? The rate is different depending on whether you're talking about the 24-year, 48-year or 100-year standards, Marmorek said, but what we basically did was look at the 2,000-odd model runs we have done and say, for those that met the standard, what was the implied smolt-to-adult rate? So you're saying that, regardless of whether conditions are wet or dry, the SAR needed to achieve the NMFS standard remains the same? Field asked. That's my understanding, Marmorek replied.

John Pisementi asked whether PATH has considered the possibility that both CRiSP and FLUSH are accurately reflecting what's going on in nature; one principally driven by inter-annual or long-term patterns, the other by intra-annual, or short-term, patterns. If this is even partially true, said Pisementi, then how do decision-makers take this information and determine whether dam removal is the only way to protect these stocks?

CRITFC's Earl Weber observed that, to some extent, both models are driven by environmental conditions. In the FLUSH model, reservoir survival is linked more or less directly to water travel time; the CRiSP model is predator-based, but is driven by a similar equation that assumes that the longer the travel time, the greater the exposure to predation. In other words, I don't think the two models are that separate, he said.

Anderson added that neither model takes into account fish condition, despite the fact that, in some years, fish are in very poor condition when they enter the hydrosystem, and in other years, fish condition is better. We have assumed that, in every year, fish condition is the same, as is the response of the fish to the hydrosystem, he said.

John McKern reiterated his point that recent passage data should be much more relevant to the PATH analysis than historical passage information, given the magnitude of the changes that have occurred in the system since the 1970s. He observed that the SARs for some coastal coho stocks, which do not have to pass through the Columbia River hydrosystem, have declined from around 6% to 0.6% in recent years; SARs for some British Columbian stocks have declined from the low teens to tenths of a percent. Is PATH considering that information, and do you plan to re-run your estimates to incorporate it, and make those results public?

Al Giorgi replied that some in PATH view the models simply as filing cabinets stuffed with information – some old, some new. Some of us are of the opinion that those filing cabinets need to be cleaned out, to make room for newer, more current information, he said; I'm not sure everyone at this table shares John's concerns, Giorgi said, but some of us do.

With respect to John's question about the coastal coho stocks, added Marmorek, one of the issues we're looking at, in terms of potential sensitivity analyses, is to say, when we project future climate conditions using the past 40 years of information, is that a reasonable assumption? One of the things we're considering, said Marmorek, is whether we want to run a new worst-case scenario that is worse than anything we've run to date.

Another PATH participant added that PATH does plan to update its analyses, not only to incorporate the more current data, but because the present analyses actually weight the historic data more heavily than the current data. There are concerns not only that that data may not be applicable to the current hydrosystem, but that there are several reports critical to that data, its research methodology, and the high level of uncertainty in those reach estimates.

**4. Transportation**.

As Jim Anderson pointed out, said Marmorek, there are linkages between the passage models and the assumptions that are made with respect to transportation. Some of the issues and uncertainties involved in these transportation assumptions include the fact that there are two components to the mortality incurred by transported fish – direct barge mortality, estimated at 2% in both passage models, and, more importantly, delayed mortality of transported fish. There is also uncertainty associated with the survival of control fish, Marmorek said, adding that the same transport:control data was used by both groups.

Marmorek then devoted a few minutes to an explanation of the delayed component or "D" factor, which he described as a very important assumption in the results. Once you get to Bonneville, he explained, you have fish that arrived there via barge, and fish that arrived there in-river. There are different amounts of post-Bonneville mortality for transported and non-transported fish. If you think about this as a ratio of survivals, he said, the "D" factor is the relative post-Bonneville survival of transported fish compared to the in-river fish.

Another way to look at this issue is in the context of the transport:control data, Marmorek continued. The transport:control data tells you the relative survival of transported fish from the point of collection to the survival of the control fish, in the years when the transport:control experiments were done. That estimate can be done through a passage survival analysis, or through the actual survival measurements from those years.

So when you take the ratio of these two estimates, you get an estimate of the relative survival of transported fish from the point of collection to Bonneville, Marmorek continued. In a given year, if the survival of transported fish was 0.98, and the survival of the control fish was 0.33, the transport:control ratio would be about 3, and the "D" value would be equal to two-thirds, or 0.66. That, essentially, is how the groups of modelers have estimated the delayed survival of transported fish. It's important to remember that there is no direct measurement of that; it's an indirect estimate, Marmorek said.

In terms of the differences between how CRiSP and FLUSH deal with the "D" value, said

Marmorek, for the historical data set, CRiSP assumes that there was a large increase in the “D” value after 1980 – that is to say, the post-Bonneville survival of transported fish improved after 1980. In FLUSH, the “D” value is relatively constant for the pre- and post-1980 periods. In terms of future projections, he continued, the CRiSP model assumes that the post-1980 “D” values are more representative of the system as it currently exists, while FLUSH assumes that the “D” values are higher than they were in the past under Alternatives A1 and A2.

What does this imply? said Marmorek. First, these transportation assumptions have a large effect on spawner projections and the ability of a given alternative to achieve the NMFS jeopardy standards, he said. In general, the future “D” estimates -- post-Bonneville survival of transported fish -- have a much more significant effect on the results than do the future estimates of the post-Bonneville survival of in-river fish. However, the transportation assumptions do not change the ranking of the various alternative actions, with A3 consistently ranked higher than A2. The only other thing to add is that, in general, the CRiSP escapement projections are higher than the FLUSH projections, Marmorek said.

Earl Weber of CRITFC touched briefly on some of the research data underlying the PATH transportation assumptions, including transport:control experiments and smolt-to-adult survival studies. In general, he said, more analysis of transportation is needed; the transport:control data are valuable for modeling purposes, but they aren’t very meaningful in terms of the likelihood of transportation’s helping to meet the NMFS jeopardy standards; smolt-to-adult returns are the most important measurement of improved stock health. The bottom line is that, while in-river survival is higher than thought, Weber said, we’re still not seeing a discernable increase in SARs.

Jim Anderson said he agrees with Weber that more analysis is needed on transportation. In particular, he said, the condition of the fish as they go through various passage routes in the system may be important. Anderson then spent a few minutes describing the evidence and assumptions underlying PATH’s “D” value calculations. He made the point that the validity of some of the “D” values PATH is using should be questioned, given the fact that there are real problems with the validity of the data for about four to six of the 15 years available. There is a lot of uncertainty about how the “D” values should be interpreted, he said; we need to look at the condition of the fish, and we need to consider different passage routes. Our work is not yet done in this analysis, Anderson said, and any interpretation of these results would be premature at this time, particularly given the fact that the estimates we’re using for “D” have a lot to do with the probabilities we’re generating for the various alternatives.

Given the fact that SARs are consistently failing to meet the minimum 2% standard, despite the fact that downstream survival may be higher than was thought, is there any empirical evidence to support the idea that improved transportation might bring us up to the 2%-6% range? asked one meeting participant. It depends on what you assume with regard to the various extra mortality hypotheses, Giorgi replied. If you are of the opinion that much of the extra mortality is related to marine effects, or to hatchery effects that are manifested in the marine environment, then transport will never accommodate that. If you believe that those types of effects will disappear over time if you take out hatcheries, or if marine conditions change, then transport could help achieve that goal.

John McKern said that, with respect to the evidence underlying the “D” factor calculations – the mortality of transported vs. non-transported fish once they leave the hydropower system – Oregon State University and the University of Idaho have been working on ways to measure post-Bonneville survival for about five years. The latest results indicate that about 79% of the transported fish and 78% of the non-transported fish survive to Astoria – in other words, there is no evidence of a significant difference in the survival of transported and non-transported fish in the Bonneville-Astoria reach, McKern said. We have no additional empirical measurements of post-Bonneville survival until these fish return as adults, at which point the ratio of transported to non-transported fish is 2:1, he said; this is not surprising, given the fact that about 50% of the in-river fish survive to Bonneville, while 98% of the transported fish survive to Bonneville, a 2:1 ratio. Why, then, would we expect to get a higher transport benefit ratio than we’ve seen? McKern asked.

In Appendix D of the Weight of Evidence report, there is a graph that compares the FLUSH and CRiSP estimates of the survival of control fish, Marmorek replied. The range of estimated survivals for control fish varies from 0.3-0.4 using CRiSP in recent years, to 0.2 using FLUSH. The point I’m trying to make, he said, is that an assumed survival of 0.5 for the control fish to Bonneville is quite a bit higher than the estimates both modeling groups came up with. I would add that it is dangerous to base a decision on a single year’s data, said Schaller – the real question is how well the analysis fits a long-term data set, representative of a broad array of environmental conditions.

## 5. Survivals of Juvenile and Adult Salmon Under Free-Flowing Riverine Conditions (Drawdown Assumptions).

Calvin Peters of ESSA provided an overview of this question, explaining that the PATH drawdown assumptions were determined by a multi-agency work group within PATH. This group identified two major types of uncertainties with respect to drawdown:

- The timing of various phases of dam removal, including the timing of the effects of dam removal on river conditions and the survival rates of fish and
- The magnitude of the effects of drawdown on fish survival rates, and what those survival rates might be through the various phases of dam removal.

The PATH drawdown work group identified four time periods with respect to drawdown: the pre-removal period, the removal period or construction phase, assumed to be two years, the transition period, following dam removal, and the equilibrium period, during which juvenile survival rates are assumed to achieve a stabilized value. The work group has identified the length of both the pre-removal and the transition periods as important uncertainties, to which various alternative hypotheses have been attached.

During the pre-removal and removal period, Peters said, survival rates are assumed to remain at their current (A1) levels). During the transition phase, survival rates are assumed to increase linearly up to their equilibrium values. Those equilibrium values are also an important uncertainty, so we have some alternative hypotheses about that as well, Peters said.

The two hypotheses about the length of the pre-removal period are three years and eight years, Peters continued; this is intended to capture the uncertainties of the Congressional approval and funding processes, as well as the potential for litigation once a decision is made. For modeling purposes, it was also assumed that the decision would be made in 1999. There are also two alternative hypotheses about the length of the transition period: two years and 10 years, he explained. The two-year hypothesis is intended to reflect the time it will take for physical changes in the river to become apparent, while the 10-year hypothesis reflects the time it will take for biological changes (e.g. predator populations) to become apparent.

With respect to the assumptions about the transition period, Peters said, we did a sensitivity analysis that looks at an initial 50% decrease in survival immediately after the removal of the dams, followed by a linear increase in survival to equilibrium levels. There are then two hypotheses with respect to what that equilibrium level might be: first, that survival will be the same as current survival through the free-flowing section of the Snake River (0.85); second, that equilibrium survival through the drawn-down reach will return to its pre-dam level (0.96).

As for adult survival rate through the drawn-down reach, said Peters, PATH has assumed that it will be 97%, based on the average adult survival assumed during the 1975-1995 period (85%) plus the average change in survival to the spawning ground after the dams went in (12%).

In terms of the implications of the drawdown assumptions, said Peters,

- Drawdown assumptions have only minor effects on jeopardy probabilities, although timing assumptions can affect the 24-year standard.
- They do not affect the ranking of actions
- Further sensitivity analyses of transition period survival and adult survival are proposed for FY'99.

There is quite a lot of uncertainty about the future effects of dam removal, both for juvenile survival and adult survival, said BPA's Jim Geiselman. There are some questions about the quality of the data used to generate the 96% survival rate for juveniles under the second equilibrium hypothesis, and its applicability to the system in the future; there are also questions about the quality of the data underlying the current free-flow survival estimate of 85%. These estimates of juvenile survival are at the optimistic end of the scale; they do not include any possible adverse effects from sedimentation or changes to predator concentrations. While we agree that these more optimistic estimates should be looked at, we feel that sensitivity to a lower range of juvenile survival should also be included, Geiselman said.

The other aspect of this analysis with which we have some concern is the length of the transition period, Geiselman continued; we feel it may take more than 10 years – the longest period included under the transition period hypotheses – for the river to return to a free-flowing condition. We have recommended that a 20-year transition period also be included. Geiselman said BPA also has reservations about the assumptions regarding adult passage through the drawn-down reach, and would like to see PATH include a less-optimistic assumption in its sensitivity analysis.

Charlie Petrosky replied that, with respect to Geiselman's points about juvenile survival assumptions, PATH's goal is to project the survival of wild, rather than hatchery, smolts through a free-flowing reach of the Snake. The historic data shows that, with only Ice Harbor Dam in place through that 140-mile reach, survival averaged about 90%; the 96% survival assumption includes the subtraction of 5% mortality at Ice Harbor. That is somewhat conservative, because the drawn-down reach would only be about half of that total, Petrosky said. Comparisons of more recent data, extrapolating from paired releases from free-flowing sections and the Snake River trap, yielded the lower bound of 85% survival. These two estimates appear to pretty well bracket the range of survival we can expect to see through the drawn-down reach, Petrosky said. Again, these assumptions had very little impact on the outcomes of the alternatives analysis. The SRP weights assigned to these hypotheses were mixed, with most SRP members favoring the more conservative hypothesis of 85%.

On the length of the transition period, the shorter two-year period does seem appropriate for spring migrants, because they transit the migration corridor during the high runoff period, and because the bulk of the sediments would be washed out in fairly short order, Petrosky continued. The 10-year transition period is of course more conservative; again, the opinions of the SRP were split, with most members leaning toward the more conservative hypothesis. In general, however, the two-year and the 10-year transition periods are considered to be fairly decent bounds on expectations.

Regarding adult equilibrium survival rates, said Petrosky, there was an alternative hypothesis presented that there would be no improvement in survival rates through a drawn-down reach of the river. The approach chosen uses data from radio-tag studies from Bjornn and others in which survival was measured from Ice Harbor Dam to Lower Granite Dam; a proportion of the Ice Harbor radio tags detected on the spawning grounds or at hatcheries was estimated. The Bjornn study doesn't really suggest that this is a survival estimate per se; it would actually be conservative for a true free-flowing system because the study can't account for all of the turnoff into smaller spawning tributaries, and lost tags are counted as mortalities, Petrosky said.

If PATH is to look at a lower future value for adult survival, Petrosky continued, that will require adjustments to the historic recruitment data; the implications to this are that the difference we now measure in adult survival between upriver stocks and downriver stocks would actually be greater, under this hypothesis, than it is currently.

Steve Hays of Chelan PUD observed that everything he heard during the morning session suggested that, no matter how many smolts were successful in negotiating the hydrosystem to reach the estuary, adult returns continue to be lower than expected, due to an as-yet undefined combination of mortality factors. I can't see, therefore, how an increase in juvenile survival through a drawn-down Snake River reach could produce the increase in SARs you're looking for, unless there is some concurrent assumption being made about this unknown mortality effect, Hayes said.

How that is addressed under drawdown depends on which of the three extra mortality hypotheses are used, Marmorek replied. Under the BKD hypothesis, nothing changes – extra mortality remains exactly the same. You get some improvement in in-river survival, but it doesn't change the extra mortality. Under the regime shift hypothesis, extra mortality is controlled by a 60-year cycle which is unrelated to drawdown. Under the hydrosystem extra mortality hypothesis, we looked at extra mortality in two ways, Marmorek said. In the first, extra mortality changes as in-river mortality changes – a 50% reduction in in-river mortality yields a 50% reduction in extra mortality. The other way we implemented it under the hydrosystem hypothesis was to say that, if the dams were breached, the extra mortality would return to the levels observed before 1970, before the Lower Snake dams were there. Schaller added that, because fewer fish would be transported if the Lower Snake dams are removed, the effects of delayed mortality on transported fish would be proportionally reduced.

**6. Spawner-Recruit Function**: **What Influence Does the Choice of Underlying Production Function Have on the Results of PATH? How Well Does the Empirical Data Fit to the Chosen Production Function?**

Rick Deriso explained that the passage models that have received considerable discussion during today's forum are essentially components of the life-cycle model, which predicts the number of adults that will return to the Columbia system, given a certain number of spawners. A classic spawner-recruit function underlies this entire analysis, Deriso said; that spawner-recruit function, developed by Ricker in 1954, is widely accepted for analysis of salmon populations throughout the world. The model has been generalized somewhat over the years; one of the issues raised in the Biological Opinion is that the model should permit depensation where supported by the data; we have done that, and have also customized the model to facilitate the in-river passage models and the post-Bonneville mortality factors, Deriso said.

Another thing to mention about the life-cycle model is that uncertainty in productivity and density dependence is explicitly considered, Deriso said – in other words, we recognize that the real world is filled with random events that occur on an individual stock basis. The model includes random, year-to-year variation that reflects the variation in data, allowing it to fit a wide range of data.

That wide range of fits constitutes another set of alternative hypotheses which we haven't really discussed so far today, Deriso continued. For example, one of the parameters in this model measures the maximum intrinsic recruitment a stock can support. That value is quite uncertain, he explained, so it has a probability distribution associated with it.

Deriso showed some examples of the ranges of possible maximum recruitments for various stocks, then talked briefly about depensation, which he defined as the possibility of recruitment failure at low spawning stock levels. NMFS is planning to do a number of additional sensitivity analyses on depensation and other elements of the life-cycle model in FY'99.

Schaller emphasized that the spawning populations PATH has looked at in this analysis inhabit a number of index areas, so that these stock recruitment curves are not merely a conglomerate of the Snake River ESU. The different index stocks used represent a broad range of habitat conditions in the Snake Basin and the lower river. In the PATH analysis, all of the individual stocks that contribute to the Snake River populations have individualized stock recruitment functions, which represent different habitat types.

Rich Hinrichsen provided a brief overview of how the data underlying the PATH spawner-recruit estimates were generated; he made the point that there are a lot of parameters that go into the run reconstruction that have not been explicitly modeled with uncertainty, as has been done with the passage model. Because the spawner-recruit data is the backbone of this analysis, he said, it would make sense for us to look at alternative run reconstructions to see what would happen if it had different parameter estimates; we should also be looking at how the probabilities of recovery and survival change with alternative constructions of the spawner-recruit data.

Geiselman said BPA has concerns about the spawner-recruitment data; there are problems with the reproducibility of the data, as well as with potential biases in the analysis due to a number of the assumptions about and extrapolations of the spawner-recruitment data. We also recognize that it will take a lot of time and effort to address those concerns, Geiselman said, and we haven't been able to address that yet – it's still on the long list of additional work PATH needs to do.

It is worth noting that a large number of researchers, working with salmon over a period of decades, have used these stock-recruitment data and have found surprisingly good relationships with environmental variables and with other stocks, said Peterman – you would not expect to see that if measurement error was a significant factor.

## IV. Results.

### 1. Juvenile Salmon Survival, Smolt-to-Adult Return Rates and Spawner Recruit Function: How Do the Results Estimated by PATH Compare to the Historical and Most Recent Empirical Estimates In Regard to Value and Pattern?

Randall Peterman began this discussion with a few general comments. First of all, he said, we have to be careful in interpreting the results when the passage models are compared to previous observations, because using historical data to try to determine whether one model is more believable than another may not be a strong test of how well those models will be able to forecast the future response of the stocks. One reason is the fact that a lot of the data are not gathered under a wide enough range of conditions, such as flow conditions which would be affected by the drawdown of the Snake River dams. The historical data sets that are being compared to the passage model results are a combination of both field measurements, which contain varying amounts of erroneous data, as well as various sets of assumptions, expansion factors, and adjustments for things like harvest rate. In other words, said Peterman, these numbers are the best we have, but they don't necessarily inspire 100% confidence. He added that the Scientific Review Panel commented that the recruits-per-spawner measure is the most important data set to use, because it takes into account delayed effects. The in-river data set does not take delayed effects into account, he said.

Chris Toole provided an overview of the PATH results in comparison with the various sets of data that were available at the time the model runs were done, and the information that has been provided by the NMFS science center to PATH. In terms of the types of empirical information you can compare results to, said Toole, we have reach survival data to which we can compare passage model results, and we have smolt-to-adult return and stock recruitment information to which we can compare life-cycle model results. He said he would concentrate primarily on the reach survival/passage model comparison in his portion of this discussion.

In the PATH report, Toole continued, what we were basically trying to do was determine the performance of the passage models relative to the available empirical information. What we have to work with is a series of reach survival estimates from NMFS studies done from 1966 through 1980, plus PIT-tag estimates from, at the time these model runs were done, 1993 through 1996.

Toole touched on some of the sampling differences between the various historical data sets, then said that, in general, the models track fairly closely with the empirical data, although, in any individual year, one model or the other will be off by as much as 10%-15% from the reach survival estimates. In other years, he said, the differences are minimal.

Toole moved on to some of the limitations to the historical data sets; notably, that there are weaknesses in Raymond's survival rate estimates; that assumptions are required to compare in-river survival estimates from the passage models, and that some of these data are not truly independent, but are out-of-sample.

Last summer, Toole continued, NMFS gave PATH some very preliminary estimates of survival rates from Lower Granite to Bonneville from its 1997-1998 studies. The issue people keep bringing up, Toole said, is, has PATH incorporated the data from these years in its analysis? That's not really as important as whether or not the models accurately predict the survival study results, said Toole. With that in mind, we ran a comparison; the results came out as follows:

| **Year** | **Observed Survival** | **CRiSP Estimate** | **FLUSH Estimate** |
|---|---|---|---|
| **1997** | **45.5%** | **46.4%** | **43.6%** |
| **1998** | **57.8%** | **52.3%** | **n/a** |

At least for 1997, said Toole, both models predicted the actual observed survival pretty well; the FLUSH estimate for 1998 is not yet available, so the jury is still out on the 1998 results. He said NMFS is still working to finalize its observed survival estimates from 1997 and 1998.

PATH then looked for an additional source of information to which to compare its passage model results, said Toole. There are no additional PIT-tag survival data available, he said, but from 1989-1992, there were PIT-tag detection probability estimates generated for the Lower Granite-McNary reach. That allowed us to do a rough comparison of our CRiSP and

FLUSH model results and those detection rates, Toole explained.

Basically, NMFS provided those detection rates, and the model attempted to duplicate them, said Toole. For 1989, the results were pretty close, but for 1990, 1991 and 1992, both CRiSP and FLUSH yielded considerably lower estimates than the detection rate estimates NMFS provided. One way to interpret these results is to say that these data are limited, and that there were various confounding factors that tended to confuse the results, said Toole. Another way to look at it is to observe that flows were considerably lower in 1990, ‘91 and ‘92 than they were in other years; NMFS has interpreted this as a reason to be cautious about passage model estimates in low-flow years.

Toole also touched briefly on the comparison of the passage model results to the Raymond smolt-to-adult return data and the available spawner-recruit data (Beamesderfer et al. 1997), saying that, for both data sets, under the Alpha model, the FLUSH model results fit the data set better than the CRiSP model results. Toole added, however, that there are serious limitations to both the SAR and spawner/recruit data sets.

Paul Wilson said that, in attempting to validate passage model results by using reach survival data, it is necessary to take into account not only how *well* they fit the data, but *how* they fit the data. The SRP recognized this, he said; one of the reasons they gave a higher weight to FLUSH is that it is a simpler model which doesn’t need as much information prospectively to predict survivals as CRiSP does.

The NMFS reach survival estimates are important, Wilson continued, but they’re not the whole story. The life-cycle model preferred by three out of four SRP members doesn’t use the absolute prospective escapement estimates from the life-cycle models – it uses the ratio of system survivals. The more appropriate question to ask is, is the ratio of prospective system survivals to retrospective system survivals reasonable, for a given management scenario? Wilson said.

One other thing to add is that, when we compare how the models fit the observed in-river data to how the combination of models, in all their parameters, fit the stock recruitment data, we get two different stories, said Jim Anderson. The weak flow/survival relationship in the CRiSP model fits the in-river data more closely, particularly in 1998, he said. Also, the observed survivals are higher than the model predictions in both cases, which is an indication, to me at least, that we have fairly high survival through the system under current passage conditions, Anderson said. When we put all of those things together into the full life-cycle model, he continued, FLUSH fits that better. What concerns me is that this is a statistical comparison of model systems that have hundreds of different parameters; I would hate to see us make a decision based only on some very simple statistical comparisons, Anderson said. As we move ahead in this process, we need to ask, what are the mechanisms underlying each of these models? One of the reasons the SRP preferred the FLUSH model is that it is really a non-mechanistic model that fits the observed data. With CRiSP, what we’ve tried to do is describe some of the mechanisms that are affecting the fish, Anderson explained. The bottom line is, we can look at this question two different ways, and get two different answers.

Tim Stearns of Save Our Wild Salmon said that, from what he has heard today, it appears that FLUSH fits better with the available smolt-to-adult return, stock recruitment and flow survival data. Can you explain why CRiSP doesn't fit that information as well, he said, what the strengths and weaknesses of the CRiSP model are, and why we would use results from a model whose results don't seem to fit the empirical data very well?

There are actually more than two models involved, Anderson replied – there are actually six or seven models that have been distilled down into CRiSP and FLUSH. The way we've done the analysis, and mixed things together in the decision process, it's very hard for me to separate out and understand the different assumptions, and how they contribute to this mixture of outcomes, Anderson said. The CRiSP model fits more closely to the observed data over longer reaches, but it is underpredicting the observed survivals from the 1990s – that is an important point, he said. The life-cycle model contains other features in addition to the passage models; for example, there is a separate set of models for the impact of transportation. To me, the key question right now is, given the current high observed in-river survivals through the system, why is the smolt-to-adult return rate only 0.25%, and what can we do to improve it? Anderson said. If we take a moment and step back from all of these models and ask what mechanisms might be altered as we do different things, then we're focusing on the real question, Anderson said. I agree with Jim, said another PATH participant – high in-river survival is only a part of the ultimate goal, which is high SARs.

As I understand what you've told us here today, said Power Planning Council member Eric Bloch, the PATH report on spring/summer chinook used all of the data that was available to it, up to 1997. As you've told us, there are some positive attributes to the old data, and some positive attributes to the new data; PATH used it all, coming up, in the process, with a means to assign weights to the old and new data, said Bloch. Some in the region seem to be suggesting that the older data should now be taken out of the mix by PATH, and given no weight at all, because it can no longer be applied to current conditions in the hydrosystem. What is the view of the PATH scientists on this question? he asked.

To start with, I don't believe the old data and the new data are appropriately weighted in the PATH process, currently, replied Al Giorgi. So it's at least a weighting issue, and it may be more than that. I have a lot more confidence in the NMFS survival data from 1997 and 1998 than I do in the estimates from the 1980s, some of which I helped generate, Giorgi said – the estimates we're getting now are simply a lot more robust, and are giving us, in my opinion, a truer picture of what's happening in the system. So you're saying that the more recent NMFS survival data should be weighted more heavily than it is currently being weighted in the NMFS process? Bloch asked. Yes, Giorgi replied.

I'm afraid this issue is being oversimplified, said Toole; while it's easy to ask whether or not the old and new data are weighted appropriately, it's not so simple to provide an answer. The old and new data are used in different places in different models in different ways; I'm not weighing in on the question one way or another, he said, but it really is a very complex issue.

**2. Efficacy of Actions for Recovery**.

Marmorek noted that, in this phase of its analysis, PATH is not attempting to identify which recovery action is best. What we did, he said, is look at the effects of the alternative management actions on the various response measures – the ability to meet the NMFS standards, for one. Other evaluations, such as the socio-economic analysis, will also play a role in NMFS' final recommendation.

In terms of the range of outcomes, Marmorek continued, as has been mentioned, the PATH analyses have incorporated many combinations of hypotheses. Looking at each of those combinations yields a frequency distribution, or a histogram showing the likelihood of exceeding a given threshold – what's the probability, say, of exceeding a spawning threshold of 150 fish 100 years from now?

It's hard to capture all of this information on a single graph, Marmorek said; one way of compressing it is to use a box plot, in which a solid line represents the median, or middle point in the distribution; a dashed line represents the mean; the end lines represent where 50% of the distribution lies, and the circles to either side represent the 20$^{th}$ and 80$^{th}$ percentiles, respectively. This box plot is intended to convey a sense of the spread of the results, which is important because of the degree of uncertainty associated with the various alternative actions, said Marmorek.

In terms of results so far, Marmorek continued, if we look at the box plots for the various alternatives, the means and medians for A1, A2 and A2' all fall below the 0.7 level identified more or less arbitrarily by PATH as a "high" probability of the likelihood of a given alternative's ability to meet the 24-year survival standard. The means and medians for the A3 and B1 alternatives are very close to, but generally above, the 0.7 line. The range of outcomes is less under the drawdown options than it is under the current operations and maximized transportation alternatives (A1 and A2).

When you look at the box plots for each alternative under the 100-year survival threshold, Marmorek continued, the pattern becomes stronger, in terms of a smaller range of outcomes for the drawdown alternatives, all of which are completely above the 0.7 level; the average probabilities for A1, A2 and A2' also wind up above the 0.7 level, although at the more pessimistic end of their probability range, A1, A2 and A2' are below the 0.7 level.

When you look at the box plots for each alternative under the 48-year recovery scenario, all of the drawdown alternatives are well above the 0.5 level, whereas slightly more than half of the hypotheses under A1, A2 and A2' wind up below the recovery level. Marmorek noted that, under this analysis, equal weights were applied to each hypothesis.

Those are the basic results, he said; the question now becomes, what happens when you apply unequal weights? What if, in other words, some hypotheses are more likely than others? We talked earlier in today's meeting about the weights developed by the Scientific Review Panel; essentially, when you apply those weights, the results are very similar to the results obtained when all of the hypotheses are weighted equally. It was surprising to us that the weighted results were as similar to the equally-weighted results, Marmorek said.

In terms of the differences between the average results from CRiSP vs. FLUSH, he continued, there is a greater difference between the average probability of exceeding the 24-year NMFS survival threshold between the drawdown alternatives and the transportation alternatives under FLUSH – there is very little difference between the two under CRiSP. For the 100-year survival threshold, the same pattern holds true; under the 48-year recovery standard, FLUSH shows an even greater disparity between the likelihood of the drawdown alternatives vs. the transportation alternatives in terms of their probability of meeting the standard. In all cases, however, the rank of the alternative actions does not change.

There are a number of ways these results can be interpreted, observed one participant. One thing we haven't really looked at is the fact that, under all of these hypotheses, you see major improvements in juvenile survival. Also, we need to bear in mind the major uncertainties associated with some of these actions, which have a significant effect on the rank of the alternatives; for that reason, it is very important to do the additional sensitivity analyses and model updating that has been discussed today. Incorporating the more recent juvenile survival information would likely yield higher "D" values, which could raise the relative performance of the A1 and A2 alternatives, and lower the values for the A3 and B1 alternatives. In addition, including some less-optimistic A3 hypotheses in the decision analysis framework would broaden the uncertainty surrounding A3, possibly lowering its scores. Obviously, he said, there is a lot more work that needs to be done before we consider these results to be final or definitive.

At the beginning of this forum, Randall Peterman pointed out that the goal of the PATH process is to look at what goes on under a large set of uncertainties, replied one of the PATH panelists. The purpose of the decision analysis is to assess the risks associated with each action, to identify the actions that are most robust over a large set of uncertainties. It's true that there is a great deal of uncertainty associated with this analysis, he said, but a high percentage of that uncertainty has been incorporated into this analysis. The strength of this analysis isn't that it tells you precisely where the mean values are in relation to whatever performance bar is chosen, he said – it is that it tells you which alternative set of actions is most robust, and yields the greatest probability of meeting the performance standard even under the most pessimistic set of assumptions, and which options are least risky. Currently, that analysis is telling us that A3 is the least-risky option.

Do the PATH panelists feel that the balance of optimistic and pessimistic assumptions is evenly distributed between the various alternatives PATH is analyzing? asked another participant.
We have accepted a number of optimistic assumptions about the current configuration of the hydrosystem and about transport without adding more pessimistic assumptions, Paul Wilson replied; for example, we assume a direct transport survival of 0.98 without any direct evidence; we also assume that there will be no increase in descaling rates in the future with extended screens, and that there will be high levels of direct bypass survival in the future. In general, he said, there are a number of areas where some of our more optimistic assumptions aren't balanced by more pessimistic counterparts.

This is an important question, added Marmorek, because it is critical to the validity of the decision analysis that the range of assumptions we explore is balanced and fair. What we've tried to do so far is to use various different types of data to make reasonable estimates that have to come out differently, he said; in other words, we have tried to anchor our range of assumptions in empirical evidence. That's why the various PATH work groups include representation from a broad array of agencies, and also include independent members whose job is to question everyone's assumptions. That process is one we want to continue as we move through our upcoming sensitivity analyses, Marmorek said.

In response to another question, Chris Toole said the 0.7 and 0.5 "performance bars" referenced by Marmorek were established by PATH; they have not been officially endorsed by NMFS. NMFS has not yet set a "bar" or standard the recommended alternative will be expected to meet; the 0.7 and 0.5 probability bars were crudely extrapolated from the Biological Opinion.

What are the chances that PATH will have the opportunity to model Alternative A3 without Idaho flow augmentation water in the coming year, and what is the likely outcome of that exercise? asked a representative from Idaho Rivers United. Basically, an Implementation Team subgroup sets the PATH analytical priorities, Marmorek replied; it has been suggested by Jim Yost that PATH look at all of the alternatives without Idaho water. That is one of 25 or 30 potential tasks for PATH in FY'99, he said; we have not yet finalized PATH's analytical priorities in FY'99. In response to the second part of your question, it's impossible to say until the analysis is done.

## V. Strengths and Limitations.

Marmorek said the subject of the various sensitivity analysis that could or should be done over the next few months has arisen frequently in the course of today's discussion; which of those sensitivity analyses will actually get done is, again, a decision that will be made in the IT arena. That leads to an assessment of what we see as the strengths and limitations of the PATH process, said Marmorek. These include:

**Strengths**

- PATH is a cooperative, multi-agency process, representing a broad array of viewpoints
- Internal review and debate lends focus to arguments and hypotheses
- All PATH work products are peer-reviewed by external scientists
- The process includes a neutral facilitator and a number of independent scientists

**Limitations**

- PATH is a multi-agency process – it takes time to reach agreement and/or consensus
- It is difficult to meet internal and external deadlines
- Resources (key personnel) are limited
- There is limited data for building models, testing hypotheses about the past, and testing

hypotheses about the future
- The modeled world does not reflect the real world with 100% accuracy
- Factors not modeled include: behavioral, productivity and genetic interactions between populations; the effects of the quantity and quality of mainstem river habitat; hydrosystem impacts on the estuary.

Moving on to possible PATH work priorities to address some of these shortcomings in FY'99, Marmorek said that, on the fall chinook front, these include refinement of PATH's preliminary analyses; on the spring/summer chinook front, they include updating the data used in the original PATH analyses, the incorporation of additional hypotheses (on transportation extra mortality and climate, for example) and the effects of the other Hs (habitat, harvest and hatchery). On the experimental management front, PATH would like to investigate the identification and evaluation of possible actions to reduce future uncertainties; on the Mid-Columbia steelhead and spring chinook front, PATH may assist in the development of a quantitative analysis to meet the NMFS requirements.

Peterman added a few comments about the interactions between the modeled world and the real world; while many have doubts about the applicability of model results to the real world, he said, the point is that all of these modeling exercises are trying to extract the key processes that are operating. I would ask you all to keep that in mind, he said. The decision analysis approach PATH is using has been shown in other fields to be the most effective way to take uncertainties and complexities into account in the policy analysis environment. People in Washington D.C. make large-scale economic policy decisions every day, decisions with huge ramifications for all segments of society, with very little analysis, compared to the depth of analysis and consideration of alternative hypotheses we're using in PATH, Peterman said. Uncertainties are unavoidable in this process, but they cannot be used as an excuse to avoid making decisions, or to delay those decisions. Otherwise, all we're doing is re-arranging the deck chairs on the *Titanic*, he said.

Jim Geiselman said that, in his opinion, while the PATH analysis does take uncertainty into account, the models are still overly optimistic, and are still under-representing the amount of uncertainty that exists. Instead of focusing on the ranges, he said, the decision analysis is focusing on the averages. Given these limitations, he said, I think the scientific review of the PATH effort needs to be broadened.

Obviously there is a lot of uncertainty inherent in this process, added another participant; there is a long list of additional items for analysis. The question then becomes: at what point do you cut off that further analysis, go with what you have and make a decision, despite the uncertainty that remains?

## VII. General Discussion.

Rob Walton of the Public Power Council read a quote from the executive summary of the 1998 PATH report, on the subject of habitat-related sensitivity analyses: "Alternative habitat scenarios lead to increases in average jeopardy probabilities for some stocks, and reductions for others.  It is at first surprising that an improvement in habitat for one stock could reduce the abundance of several stocks.  There is, however, a logical explanation: when habitat improvements lead to larger escapements for stronger stocks, this triggers higher in-river harvest rates for all Snake River stocks, including the weaker stocks, since in-river harvest rates increase as total Snake River abundance increases.  As a consequence, all stocks are harvested at a higher harvest rate, which can lead to lower escapements than would otherwise be the case.  This is particularly true for weaker stocks in pristine habitats."

If in fact the goal is to maintain and recover salmon stocks, particularly the weaker listed stocks, why did PATH assume a harvest regime that would lead to lower escapements, particularly for the weaker stocks? Walton asked.  We're basically using the harvest rules that currently exist, Marmorek replied; we could certainly explore a different assumption, but the question is, how can you tell a Marsh Creek fish from a Bear Valley Creek fish as they pass Bonneville, and adjust your harvest rates accordingly?  That's the weak-stock problem, Marmorek said.

I was recently at a public meeting where PATH's work was characterized in the following statement, Walton said: "Evidence [from PATH's work] supports assumptions that the hydropower system, and not harvest, habitat degradation and hatcheries, most likely has caused much of the decline in Snake River spring/summer chinook."   In your view, is this a fair characterization of what PATH has said? Walton asked.

When we looked at the historical information on spring/summer chinook, Schaller replied, harvest levels on that stock have decreased over the years; even though this is the case, the escapement of spring/summer chinook has remained depressed.  That was one of the conclusions from the retrospective analysis, he said.  Marmorek added that all of the hypotheses consider some deterioration in climate and ocean conditions since 1977, which might be encompassed in the "habitat" portion of that statement.

Carmen MacDonald of Salmon, Trout and Steelheader Magazine observed that it is important for any scientific process to have some degree of confidence associated with its results.  From what I've heard today, MacDonald said, there is a polar opposite of opinions in this room about the validity of the PATH results.  At what point, given the fact that the listed populations continue to decline, do you reach a confidence level sufficient to take action?  That's a policy question, which has to do with the level of risk the decisionmakers are willing to accept, Geiselman replied.  There are many other facets to this analysis that have not been discussed today, including the socio-economic analysis; all of that will have to be taken into account before the decision is made.  You can certainly reach a point of diminishing returns as you analyze and re-analyze the information in an effort to reduce uncertainty, added Peterman, and at that point, it's time for the decisionmakers to step in.

Another participant made the point that it would be more accurate to refer to "drawdown" as "dam breaching;" to call what is being contemplated under Alternatives A3 and B1 "drawdown" is misleading to the public.  We can certainly try to be more precise and consistent, agreed one PATH participant.

Tim Stearns of Save Our Wild Salmon said that, in the 20 years he has been following salmon-related issues, the PATH effort is undoubtedly the most rigorously structured and peer-reviewed process that has ever been done in the basin. It has obviously been a very difficult, incredibly work-intensive process, he said, and I wanted to thank you for all of your efforts. In terms of specific comments, he said, I view models as a way to organize and draw conclusions from large volumes of data. It seems to me, as a former math teacher, that we've probably tortured the body of spring/summer chinook information to death, while the early results you've provided from your fall chinook analysis are obviously much more robust, and give a much clearer answer. The PATH analysis has shown some fairly optimistic modeling results, across a wide range of assumptions; however, I don't see much reason for optimism in the adult returns we've seen in the past few years.

Giorgi replied that the fall chinook analysis is in an extremely preliminary form at the moment; another PATH participant added that there simply isn't as much data on fall chinook, so that analysis will not be as robust.

Another participant observed that there may be a fundamental disconnect between the real world and the PATH analysis, given the implication that factors like the general decline of all Northwest salmon stocks, including coastal coho and Puget Sound chinook and steelhead, and the inexplicably low SARs in recent years despite the fact that juvenile survival is higher than many have thought, can somehow be remedied by breaching the four Lower Snake dams. I would say, as a PATH participant, that I am as skeptical as you are on many of those points.

Marmorek added that PATH does take ocean conditions into account, and has considered scenarios that assume ocean conditions may continue to worsen. Basically, no one in PATH is saying that the conditions you describe are all attributable to the four Lower Snake dams, he said. I don't think we're blind to whatever Achilles heels the analysis may have, Marmorek said.

Having followed PATH for some time, said another participant, I'm impressed by the fact that there is still considerable confusion about what exactly PATH is doing, about the implications of the results to date and about the issues and uncertainties you're facing. Are there any plans to issue some sort of document that will put those implications, issues and uncertainties into a form that is easily understood by the general public? When someone says, "what you wrote isn't clear," that isn't quite enough direction, Marmorek replied. We tried to write an executive summary which was clear; if parts of it are unclear, then we need to know what parts you're talking about. Certainly we think it's important to make everything we do as digestible as possible.

Silverberg thanked everyone for their input at today's forum, and asked that everyone consider what further dialogue and opportunities for public discussion on PATH may be appropriate. With that, the meeting was adjourned.